Author's response to reviews

Title: Childhood mortality in the Democratic Republic of Congo: Cross-sectional evidence of the effect of geographic location and prolong conflict from a national household survey

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Author's response to reviews: see over
Dear Dr Rebecca Grais,

Please find enclosed our revised manuscript, which addresses the reviewers’ concerns and suggestions. What follows is a point-by-point rejoinder to the comments provided as part of the review process. Each group of responses has been numbered to correspond with those on the comments. In addition, in the revised manuscript we have highlighted in red the areas that have been modified compared to the original submission.

We would like to thank the reviewers and the Editor for thoughtful comments and suggestions. We are truly delighted that the Editor has given us the opportunity to respond and improve our manuscript and we hope that it is now acceptable for publication in the BMC Public Health.

Reviewer: Matthew Coldiron

Reviewer’s report:

Major compulsory revisions:
1) The article is much too long, and the English is of variable quality depending on the section. The introduction should be half as long, and the methods section should also be streamlined.

Reply: Following the reviewer’s comment, we have revised both the introduction and methods sections. The manuscript has been shortened by reducing the introduction by half and removing the appendix. It now contains no more than 24 pages from the original 32 pages. The manuscript has also been edited by a professional copy editor.

2) The authors should strive to use more neutral descriptions of the long and complex conflict in the DRC, the specific details of which (however interesting and important) are not relevant to their analyses.

Reply: We thank the reviewer for this important observation. Following the reviewer’s suggestions, the paper has been revised to deal satisfactorily with his suggestions.

3) The discussion of potential weaknesses in the DHS sampling methodology is valid, but too long, and inappropriately placed in the Methods section. - This could be included in the Discussion section.

Reply: We have removed the section on limitations in the methods section and have inserted it in the discussion. The limitations section has been also shortened as suggested.

4) The title does not accurately reflect the results presented. I realize that the most complex parts of the analyses related to the spatial effects, but most of the results presented actually pertain to demographic characteristics. I believe this warrants mention in the title.
Reply: The reviewer is right, the results of our paper emphasise on the spatial effects of mortality in the DRC. These have not been looked at previously and may be surrogate of many environmental factors including the on-going conflict. We have revised our results section to highlight the spatial effects and slightly changed the title to reflect these changes.

5) Most importantly, I disagree with the author's statement justifying the added value of their results - that they will help provide recommendations for humanitarian intervention and policy formulation. While interesting in theory, and impressive in terms of the statistical analyses, I see zero added values for planning humanitarian intervention or informing policy. The simple fact is that most provinces in the DRC are the size of countries, and have great variation – in terms of access to care, accessibility in general, and certainly in terms of active conflict. Doing this analysis at a provincial level is a gross oversimplification of the complexity of the current situation in the DRC, and I see no potential implications on policy.

Reply: The reviewer makes an important observation here. Such a spatial analysis should preferably apply to small scale geographic units such as villages/zones/districts using for example census data, where the precision of the spatial analysis would be much higher given the fact that provinces in DRC are a huge unit. Unfortunately, DRC conducted its last census in 1985 (30 years ago) and most DHS data or censuses do not collect data on child mortality at the small scale geographic unit and often the full dataset is not available for such analyses. Hence, analysis of the household survey data provided by the DRC DHS at the province-level is the only feasible way to evaluate spatial variation of mortality in DRC.

However, an under-five mortality rate in DRC is known only for the overall nation. This masks considerable variation across provinces within the country. By contrast we examined mortality rates for each of the 11 provinces of DRC. As the reviewer rightly pointed out, we recognise that the province is still a large unit. However, the disaggregation to this level represents a considerable advance over the use of a national prevalence of the under-five mortality rate and our analysis provides the first province-level information on childhood mortality risks. More importantly, these spatial mortality risks take into account the environmental impact of the province of residence for the child (environmental impact of on-going conflict).

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal
Quality of written English: Not suitable for publication unless extensively edited
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
I declare that I have no competing interests.
Reply: The reviewer will see that the article will retain sufficient scientific interest which should warrant its publication. We thank the reviewer for this important observation. We have taking into account all of the reviewer’s comments and suggestions. The manuscript has undergone an extensive copy-editing by a professional copy editor.

Reviewer: Samuel Manda
Reviewer’s report:

The authors use geo-statistical methods via simulation based Bayesian methods to model regional spatially correlated effects on child mortality in the Democratic Republic of Congo (DRC). They use a nationally representative data from a DHS survey of 2007. They implement the fitting of the model in a Bayesian package BayesX. In general the article has a purpose in what it wants. However, I have some comments as I outline below.

Reply: We thank the reviewer for this kind observation.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
- Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1) Some syntax/grammar; page 1 line 1, I think “sensitive” is misused here; maybe important or critical etc. Avoid using the word “the” when things are general e.g. “The investigations.” just say Investigations …; The infant, just say infant; The childhood say Childhood; unless we know what infant mortality you are referring to in later sections

Reply: Following the reviewer’s comment, the manuscript has undergone an extensive copy-editing by a professional copy editor.

2) Avoid long sentences, e.g. page 1; Within SSA …. Poor governance is a very long sentence

Reply: See our reply above. The manuscript has undergone an extensive copy-editing by a professional copy editor.

3) Many instances of the above, take time to proof-read

Reply: See our reply above. The manuscript has undergone an extensive copy-editing by a professional copy editor.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1) The abstract, has not data pointing to the significance of the war on child mortality even though most of the Introduction Section has war-conflict as the main theme. In the conclusion section of the abstract, the authors point out that under-five mortality unexplained by on-going conflict and factors. I thought some factors explain this mortality as explained in the results section? But on the other hand, mortality has declined. Have you considered displacement of mothers to safer areas, and its impact on “left-over” mortality etc? You have touched on data quality issues in the affected areas, but how has this impacted on the mortality and associated correlates?

Reply: We thank the reviewer for this important observation. The reviewer is right some of factors considered in this analysis have been able to explain the risk of under-five mortality. We have revised the abstract and results section to clearly state the study findings. On the other hand, we also agree with the reviewer: mortality has declined perhaps as results of displacement of mothers to safer areas, which we did consider in this analysis and its impact on “left-over” mortality. This study is not able to assess impact of displacement on the mortality and associated correlates because of the lack of displacement information in the DHS data but we are only able to highlight the apparent decline of mortality and associate it to data quality issues in the affected areas.

2) It’s clearly that DRC has been going a rough time in terms of the devastating civil wars in recent times; added to this is the fact that generally countries in the sub-Saharan countries have poor child survival outcomes. These later are well documented. Maybe the authors might want to start from discussing the well-known effects of maternal social-economic factors on child survival, and then discuss the added burden created by the wars.

Reply: The reviewer makes an important observation here. In fact, our paper argues that the on-going conflict in DRC alone cannot explain higher under-five morality rates observed in DRC. In this way, our analysis accounted for the impact of the on-going conflict by controlling the geographic location where the child lives (see on page X about our comments on the above observation. We have also included in Table 2 estimates of under-five mortality in neighbouring countries of DRC to put DRC’s mortality estimate into context.

3) There is some factual errors; the title has novel evidence; I am not sure that the methods are novel; they have been used so many times; indeed the first author has used these in similar situation in SSA region. The authors went on in the discussion section, page 18 to say most studies in SSA failed to control for geographic location. Maybe the authors are not aware of other studies in SSA that have controlled for geographical effects on child mortality

Reply: We removed the word novel from the title. The title now reads “Childhood mortality in the Democratic Republic of Congo: Cross-sectional evidence of the effect of geographic location and prolong conflict from a national household survey.” However, the point we wanted to make here is the fact that apart from a few studies (Sartorius et al. 2007, etc...) these methods have not been used widely in the analysis of mortality data in SSA and the sub-optimal use of these methods warrants the deserved attention in this paper because of the relevance of spatial effects on mortality outcomes in SSA.

4) So both non-conflict provinces (e.g. Kinshasa) and conflict provinces (e.g. North Kivu) have lower child mortality rates. The reasons given are that the first has some functioning health infrastructure; the latter has several NGOs. So these variables account for variations in child mortality more than conflict? Are these not found in the other provinces? Maybe the war is affecting all the regions, so that one cannot really differentiate war-related variables?

Reply: This is a very interesting observation from the reviewer and we agree it is probable that the on-going conflict is affecting all regions so that one cannot differentiate war-related impact on under-five mortality. We have added the reviewer’s suggestion in the discussion.

5) On the results section, some statistics on mortality are presented as % such 207.8% and 184.8% as if more deaths than children exposed? Then later the statistics are presented as number of deaths per 1000 live births. Provide a clear and concise definition of under-five mortality (the number of death within five years out of 1000 live births).

Reply: The text has been revised accordingly.

6) The first two paragraphs on page 9 are out of place and must be in the Discussion, which itself is quite long at present. So some editing would be needed in the discussion

Reply: The text has been revised accordingly and the discussion shortens as suggested.

We would like to thank the reviewers for thoughtful comments and suggestions. We truly appreciate your interest in our work. We believe that as a result of the review process our paper has greatly improved and hope that it is now acceptable for publication in BMC Public Health.

Yours sincerely,

Nganga-Bakwin Kandala, PhD